

301

THE
**BOSTON MEDICAL AND SURGICAL
JOURNAL.**

VOL. VI.] WEDNESDAY, AUGUST 8, 1839. [NO. 26.

BARON CUVIER'S LAST SICKNESS.

THE last illness and death of Cuvier were attended with some very remarkable circumstances. On Monday, the 7th instant, he complained to his friend, M. Duméril, for the first time, of an uneasiness at his stomach, with looseness of the bowels, for which he was advised to keep his room next day, and to take some remedies suited to the circumstances. This he declined doing, because he had to preside at a council of state, a duty which he accordingly discharged. On his return, he met with the physician who usually attended him, M. Allard, and whom he took the opportunity of consulting. Cuvier had experienced at breakfast, that morning, a considerable difficulty in swallowing; in consequence of which, M. Allard thought it necessary to advise the application of leeches to the anus. On returning home, however, his patient betook himself to his scientific labors, in which he remained engaged from two o'clock in the afternoon till half past five, when he was summoned to dinner. It was only then that he informed Madame Cuvier of M. Allard's having prescribed for him, intimating his intention of postponing the application of the leeches till his usual bed time—being at that moment, he said, more inclined for his repast. He sat down accordingly to dinner, but scarcely had been helped to some soup, ere he found that he was almost entirely unable to swallow. M. Allard was now sent for, as were MM. Duméril and Orfila. Leeches were applied in the evening, and in the course of the night he was bled from the arm; but neither of these means was followed by any relief. Next day, MM. Dupuytren, Bielt, and Koreff, were added to the attendants, and joined them in consultation. On Wednesday morning, it was resolved to administer an emetic, and four grains of tartarised antimony were dissolved in some spoonful of water; but only a small portion of this could be got down. At this time, M. Cuvier had no appearance of illness; he had no uneasiness, and no fever, but merely complained of extreme difficulty of swallowing, and of a gradually increasing inability to move the upper extremities, without there being the slightest perceptible change in these parts. The emetic did not act upwards, but produced several evacuations from the bowels. Twenty-four grains of ipecac. were afterwards injected into the stomach by M. Dupuytren, by which, however, no effect was produced; and some hours afterwards, four successive doses, of twelve grains each, were similarly introduced,

but without any emetic effect resulting. The difficulty of moving the hands and arms went on gradually augmenting. Leeches to the upper part of the spinal column were now recommended, and next day (Thursday) cupping-glasses were applied along the back, but scarcely more than four ounces of blood were obtained. The upper extremities were now perceived to be slightly tumefied. Various plans were suggested in consultation, particularly the actual cautery, and blistering to the spine—the latter of which only was adopted, and even that, it would appear, but imperfectly. By Sunday the paralysis had extended, and in fact had become general, implicating the respiratory muscles; the lower extremities were absolutely motionless, and the stomach and bowels seemed to have ceased to perform their functions. M. Cuvier, however, retained his mental faculties unimpaired, and was fully aware of the danger of his situation—constantly expressing his regret at leaving so many works unfinished. "This hand," said he repeatedly, "this hand, which has performed so many dissections, and committed so many of the results to paper, is henceforth condemned to inactivity." Afterwards, as the symptoms continued to increase, he spoke, not merely of the loss of his limb, but of his death, and expressed, with deep regret, the necessity of leaving his *Comparative Anatomy* incomplete: in fact, he was engaged in remodeling this great work at the time he was stricken with the fatal paralysis. On Sunday, at five, P. M., he spoke with difficulty, and only to express the general uneasiness which he felt. M. Duméril, when he quitted him, remarked that he would see him again early next morning; but Cuvier shook his head in such a manner as to express his belief that they should meet no more. In a few minutes after this interview, he began to sink, and expired between ten and eleven at night.

The general impression among the medical attendants was, that M. Cuvier's complaint depended upon some lesion of the spinal marrow, attended with pressure; and this expectation was the more probable, as it appears that their patient, not long before his death, had met with a fall, in which he had strained himself in the effort to recover his balance. The body was examined on the 16th, when the following appearances presented themselves. The general aspect externally remained unchanged, except that putrefaction had already made some progress; the cranium was very large; the vertex flattened, and almost quadrilateral; the thickness of the parietes, particularly at the frontal sinuses, was very great; the right side projected more than the left, and the same was observed of the parietals; the occiput was also fully developed; the inner table of the frontal bone exhibited three nipple-like projections; the convolutions of the brain were very numerous and voluminous. The lateral ventricles were very much dilated, and contained a small quantity of reddish serum; the membrane lining them was slightly rugose; there were some calcareous depositions in the pineal gland; the origins of the nerves were sound, and the whole of the encephalon and spinal cord were without any alteration in texture, color, or form. The oesophagus presented no appearance of disease, and all the viscera were sound. The processus dentatus was unusually large, and there existed a sort of bony projection at each point of junction between the

vertebræ, along the whole anterior surface of the column. These, however, must have been long present, and cannot be regarded as connected with his death: indeed, they were supposed by the reporters, and with some probability, to be connected with the peculiar habits and gait of M. Cuvier. The entire absence of any post mortem appearance calculated to explain the phenomena attending M. Cuvier's death, has led some—particularly Magendie—to hazard the opinion that his illness was, in fact, but a modification of cholera, indicated in the first instance by diarrhœa, and subsequently by nervous depression, the intellect remaining unimpaired.

Sœmmering, as the extreme weights of the healthy human brain, gives two pounds five ounces and a half, and three pounds three ounces and three quarters—the great majority being intermediate between these two; and M. Bérard, in some recent examinations, has arrived at nearly the same results. But the brain of M. Cuvier weighed three pounds ten ounces four drachms and a half, being much above the extreme weight mentioned by Sœmmering. Besides, the cerebellum and tuber annulare were compared with those of a male adult, and found to exceed them in weight only by a drachm and a half; so that, in M. Cuvier, the excess was almost wholly confined to the extraordinary development of the anterior lobes; that is, to the organ of the intellectual faculties. Again: according to M. Desmoulins, one of the characters of the brain, with which superiority of intellect seems to be associated, is great extent of surface, resulting from the number and depth of the convolutions; so that a great expansion of this kind might be comprehended within a cranium of moderate dimensions. Viewed in this light, the brain of M. Cuvier was even more remarkable than with respect to its size—none of the distinguished anatomists, who were present at the examination, having ever witnessed convolutions so numerous, or anfractuosities between them so profound. It was at the upper and anterior part of the brain that this conformation was most strikingly developed.—*Medical Gazette.*

THE NEW YORK PRIZE ESSAY.

Further Remarks on the New York Prize Essay, in a Letter to the Editor of the Boston Medical and Surgical Journal.

SIR,—I beg leave to tender you my thanks for the general typographical accuracy with which my communications have appeared in your interesting periodical, and could wish them more worthy of the public regard. There is, however, one small error in my remarks, which appeared in your Journal of the 11th instant (p. 350), which I deemed of sufficient importance to notice. The error is of one letter only—a *v* for an *r*. *Pleno vivo* is printed *pleno vivo*. This term occurs in one of my extracts from the Prize Essay of the *New York State Medical Society*, in which I aimed at perfect correctness.

Pleno vivo—literally a *full river*—is probably meant, by the writer of the Essay, for a *full stream*. The author, however, and his elevated

patrons, are entitled to the privilege of having their terms literally construed, if they choose. *A full river of blood*, drawn from a patient in typhus fever, will perhaps best suit their ideas of its treatment, which is, at any rate, that of drawing blood, either by venesection or by arteriotomy, till faintness is produced, which is to be aided and kept up by large enemata and drastic cathartics. For the latter purpose, calomel and jalap are repeatedly recommended.

It will not, I hope, be deemed improper for me, in this place, to make some further remarks upon this very extraordinary Essay.

The views and doctrines of prize publications are given to the public with far more than ordinary tokens of notoriety and consideration. They are ushered into the world as containing not only the very deliberate sentiments of the writer himself, but also of the Committee, College, or Society, which approves and patronises them. They are the results of successful competition, and the productions upon which rewards, both honorary and pecuniary, are bestowed. When published, they become public property, and, if correct, ought to be received and regarded as grand luminaries, placed aloft to guide the benighted, to instruct the inquiring, and to confirm the doubtful. And never—never, in any case, ought they to pass unheeded, or to roll down the stream of time unnoticed; especially when they are upon topics so immensely—I might have said infinitely—important, as those of the lives and health of the community. Indeed, they ought to stand as enduring monuments and mementos of the periods of the world in which they issue, and as marks of the sentiments, opinions, and doctrines, of particular eras, particular places, schools, and celebrated men; and all this, because we are to suppose their principles fixed on by the wise, discerning, and philosophic, after deliberation the most mature, and scrutiny the most severe. Shall this pamphlet, then, pass without animadversion, which makes HIPPOCRATES, and the most celebrated names of antiquity—as well as those of Huxham, and Cullen, and Pringle, and Rush, and Good, and Warren, and North, and Todd, and Miner, and Tully—a parcel of dunces? which asserts the innocent nature of the most nauseous animal putrefaction, and that cleanliness and ventilation are of no utility in preventing fevers? that the best mode of treating typhus, in all its forms, instead of being that of moderately evacuating or mildly strengthening the feeble patient, is to bleed *pleno vivo*, and to purge with drastics *ad infinitum*?

How very discrepant are the notions of the prize writer and his patrons, from the philosophers of the profession above enumerated; as well as from those, although not of the medical vocation, who yet had minds of sufficient magnitude to throw a gleam of light upon every subject on which they glanced! Lord Bacon, for instance, ranks the smell of a prison, where the inmates have been long kept in a state of filth and impurity, as next to the virus of the plague. And Erasmus ascribes the spreading and mortality of the plague itself, to the want of cleanliness, and to animal filth “within doors.” It would indeed appear, that, in the time of Erasmus, the inhabitants of London, and the English in general, were the most slovenly people in Europe; whereas they are now the most cleanly. And to this last national virtue,

their immunity from the plague, and the inconsiderable progress of the cholera, may be attributed.

I had almost thrown down my pen in disgust, when I recollected that I was advocating what was so universally assented to by the wise, the good, the scientific, the philosophic, the moral and religious, of all ages and of all nations. But I recollected again, that I was actually controverting a doctrine sanctioned by the *New York State Medical Society*! and that so lately as 1828!

What an immense deal of pains have Philadelphia, New York, Boston, and our other cities, been taking, to clear away exuvie from their streets, for nothing at all! Why had they not referred to this prize pamphlet of the magnanimous *STATE Society*, which tells us, after quoting some instances of escape from being poisoned to death by stench and putridity, that "these facts are considered sufficient to prove that fever of no kind or description ever arises from animal putrefaction" [p. 50]; and that ventilation does not contribute to health, is proved by its opposite not producing typhus fever, nor any other form of disease? To establish these points, the case of the Russian boor, who lives with his family in a hut under ground, which is made tight by caulking with moss, and which for six months is not ventilated, is adduced as triumphant proof.* But the writer shall here again speak for himself. He says—"From these facts, we learn that the effluvia, arising from the crowding of healthy persons into close, unventilated situations, in cold countries at least, is not productive of fever; and, upon examination, we shall find that the same cause is equally innocuous as regards the production of typhus fever in warmer regions of the globe" [p. 52].

Of sweating, which is considered, by all who know anything of spotted and typhus fevers, to be of the utmost importance, and by some as the *ancora sacra*—a remedy, indeed, which has given celebrity to the name which first introduced it, and a process which arrested the mortality of those fevers, which before were sweeping to the grave almost every individual who was subjected to an attack; of sweating, then, the prize writer speaks in the following terms:—"This destructive and irrational practice is fortunately now almost exploded at the present time; and physicians now, with the exception of using a few antimonials, generally leave it to nature, who effects it best in her own way" [p. 8].

As might have been expected, bleeding is already adopted, by those who bleed in typhoid diseases, even in the cholera itself! Broussais bled Casimer Perrier with leeches, and Mr. Grimm, of New York, was subjected to phlebotomy: both are dead. Dr. Nelson also tells us of its having been adopted in Montreal, but with such ill success as to confound its most strenuous advocates. From late accounts from Canada, it appears that typhus fever there follows the cholera, as a modification of that epidemic, but as a less dangerous disease. Should, however, the lancet be introduced into its treatment, it may be easily made equally mortal. The inconsiderable mortality which attended the spotted and typhus fever at New London, the present year, may be attributed to the unanimity of the physicians there, headed by Dr. NORTON, in adopting

* It ought to be here recollected, that fish will not poison hollies made up of that material.

an appropriate mode of treatment, viz. : a supporting, exciting, stimulating, and sudorific plan, and in judiciously abstaining from bloodletting and all other debilitating remedies.

As I have stated that the prize pamphlet will not admit animal putrefaction, nor animal effluvia, nor want of ventilation, to produce typhus fever, it may be inquired, by those who have not seen the Essay, to what cause the writer does impute it? I answer : to the same vegetable miasm which produces intermittent and remittent fevers.

Now, that he is here again egregiously incorrect, is fully proved by the late PROFESSOR SMITH, of New Haven. DR. SMITH, in his "Practical Essay on Typhus Fever," says that "It has been suggested that typhus occasionally arises from marsh miasmata—the same which, under certain circumstances, produces intermittent and remittent fevers. A fact, which I shall here advance, is strongly opposed to this hypothesis. On the Connecticut River, from Northampton, in Massachusetts, to its source, a distance of more than two hundred miles from north to south, and on all its tributary streams on both sides, for an hundred miles in width, there has been no instance of any person's having contracted the intermittent fever, from the first settlement of the country to the present time; and yet the typhus fever has prevailed more or less in every township within that tract of country" [p. 15]. Dr. Smith's Essay was published in 1824.

I shall conclude with a few remarks on bloodletting. This operation in congestive diseases is at best a hazardous experiment.

In cholera, spotted fever, and pneumonia typhoides, the blood leaves the smaller vessels, and, from debility, quits the capillary system—weakness becoming first apparent and alarming in the weak and small vessels. The veins are flat, and the pulse frequent and feeble; whilst the large vessels near the heart, with the heart itself, its auricles and ventricles, as well as the pulmonary system, are engorged with a superabundance of the vital fluid. Now, could bloodletting reach this engorgement, it would, to be sure, answer one remedial purpose. But will it do it? I fear not. By letting blood in such cases, you stand the very great chance of doing a certain injury for an uncertain good. You with absolute certainty draw blood from a vein where more is needed, with the great hazard of not relieving the parts which are suffering from congestion. The mechanical or hydraulic principle upon which your operation of phlebotomy is founded, cannot, with any degree of certainty, be relied on. You bleed, not because there is too much blood in the vein or artery which you open, but because there is *too little there*, but *too much* somewhere else. But why is there too much blood in the heart, lungs, and large vessels? There are at least three reasons or causes for this:—1st, the brain acts feebly on the heart; 2d, the heart acts feebly on the blood; 3d, the minute vessels are affected with debility and consequent spasm; and a 4th reason may be, that the pores are obstructed. The pains in cholera, spotted fever, and pneumonia typhoides, are all spasmodic. They are relieved by opiates, by ammonia, by ether, by oil of amber, by camphor, by warmth, and other antispasmodic remedies; they are increased by cold, by depletion, and by whatever debilitates the system. To relieve congestion, therefore, we remove the

spasm, we relax the pores, we excite the stomach, the brain, the heart, the arteries, the nerves, and the lungs, by suitable diffusible stimulants. We thus enable the heart to push forward its load of *black blood* into the small empty arteries and collapsed veins; we oxygenate it in the lungs, by the admission of pure air, and by well ventilated, but not too cool, apartments. We thus increase the energy of the whole debilitated system, and quiet the distress, and warm the surface, and raise the pulse, and brighten the eye, and procure sleep, and bring comfort and hope to the afflicted patient and his weeping friends. Our methods of treatment, in fact, reverently to use a sacred metaphor, are to draw with the cords of love, rather than to drive with bolts of thunder.

Still, we fully agree, that in cases of croup, in genuine pneumonia, and other inflammatory diseases, which are indicated by a full, tense pulse, by prominent veins, and by a dense fibre, we bring the same relief, and the same cheering results, to the patient and his friends, by the free use of leeches or lancets, and the liberal abstraction of blood.

There is, however, a small, depressed pulse, with a tense fibre, which admits of some bloodletting; and the same remedy is appropriate, sometimes, to a greater extent, in some cases of mania, wherein the pulse is natural, but in which there is a morbid strength in the muscles, an insensibility to cold, and no diminution of appetite. Still, the general system is in a very different state, in such instances, from that of cholera and other typhoid diseases. In the latter, there is a soft, velvet-like feeling of all the muscular parts; a soft and easily-compressible pulse; a shrinking of the features, and a frequency of pulsations, which absolutely prohibit the lancet.—Yours, &c.

Lebanon, Ct. July 23d, 1832.

JOSEPH COMSTOCK.

CHOLERA IN NEW YORK.

On the Symptoms of Cholera in New York; with some Remarks on the Management of the Disease. Read before the Boston Medical Society July 23, 1832, by JOHN WARE, M. D.; and communicated for the Boston Medical and Surgical Journal by the Publishing Committee.

THE object of this communication is to give some notice of the symptoms of Cholera, as they were presented to the writer during a short visit to the city of New York, since its prevalence there, and some suggestions with regard to the management of the disease.

A few days' opportunity only of observing so formidable a malady, may be thought hardly sufficient to justify an individual in offering anything concerning it to his medical brethren or to the public. But it is to be recollected, that in the case of a new disease, we are all obliged to approach it in a state of at least partial ignorance; he, therefore, who knows but little from actual experience, may be able to impart something. His labors are still more likely to be useful in preparing others for its attack, if it present a variety in its aspect, as it appears in different places; if for instance it differ in Canada and New York, from the description we have had given of it in Asia and in Europe.

This appeared to be the case. At least the impression made by the inspection of patients laboring under Cholera, was different from what the usual descriptions given of it had prepared me to expect. Not that there was any variation in the symptoms or course of the disease, which could for a moment throw any doubt on its identity; but there was a very considerable variation in the relative prominence of the phenomena.

So familiar have we become with accounts of Cholera, that, instead of describing the disease, as it presented itself in detail, it will be only necessary to refer to those symptoms concerning which some particular remark occurs. The most universal of the symptoms, were the deadly coldness of the whole surface of the body, and the soaked, sodden and shriveled appearance of the hands and feet. Generally, also, there was a bluish or livid tinge of the skin of these parts, particularly of the extremities of the fingers beneath the nails. The hand in some instances resembled that of a person who had been working in a black dye. The blue or dark color of the face and of the rest of the body was not very common; much less so, according to the information of those who had visited Montreal, than it had been there. Still, a few patients were seen so dark, as not to be readily distinguished, across the ward in which they laid, from mulattoes. The cold tongue, which has been described as so strikingly characteristic in some places, was noticed in but a small proportion of cases. It had generally a slight white fur. The pulse, though commonly very small and obscure, was not so frequently extinct at an early period of the stage of collapse as was expected; and in some cases it was found quite distinct and of good volume, at not a very long period before death.

The vomiting and purging were less violent and frequent than had been expected. It was rare to witness more than one or two patients suffering from either, during a visit to a hospital containing twenty or thirty. They seemed to take place chiefly in the earliest period of the case, and in some, never existed in any violent degree. One patient was seen at 11 A. M. whose bowels, according to his report, had been in a regular state the day before, and who had only had, in the course of the morning, three or four discharges in the privy. He had not vomited at all; yet he had scarcely any pulse—his skin and tongue were cold, and his hands and feet were affected by cramps. He had regarded himself as being well on rising in the morning.

Nearly all the subjects of the disease complained of cramps, or said that they had been afflicted by them when first seized. Still, very few were seen laboring under them so severely as to occasion any marked external demonstration of suffering. In nothing, indeed, did the cholera of New York differ more from the most common descriptions, than in the absence of any indications of great distress. It is true that a few seemed in much agony when vomiting, or undergoing spasm; others complained bitterly of thirst and oppression, and burning at the stomach: but in general there was little complaint, and little disposition to notice external objects at all. The aspect of the patients was almost that of indifference, and unconcern as to the event in themselves and in others. They seemed like persons totally absorbed in their own sensations, although in sensations which were not of a very acute or distressing

character. They appeared often as if in a benumbed or stupefied state ; yet were without difficulty roused, and were at once perfectly sensible. Neither was there the sulkiness or irritability which has been said to appertain to patients with cholera. They answered questions readily and pleasantly. Though surrounded by medical men, and undergoing frequent examinations, I saw no instance of the manifestation of ill humor. Often, as soon as a physician approached the bedside, the tongue would be protruded and the arm stretched out.

In some cases the peculiar hollowness of the countenance, and the shrinking of the body and limbs, were strongly marked ; and in these, the aspect of the dying person was almost terrific. But these appearances were frequently wanting even in bad cases, and I am not aware that the countenance could have been always distinguished with certainty from that which is exhibited in many other severe and exhausting diseases. Neither did the voice vary essentially from that of patients with such diseases. The shrieks and cries of pain, which accompanied the vomiting and spasm, were perhaps a little more characteristic.

The respiration was not always accelerated ; but generally at least was performed without much action of the diaphragm, and was consequently attended by considerable heaving of the chest, and some labor. The chest seemed also as if imperfectly distended. In the act of death, this mode of respiration was continued ; it simply became less and less full, till it ceased altogether. Only one individual was noticed, in whom death took place with a kind of breathing like that so commonly witnessed ; viz. that accompanied by the rattling of mucus in the throat.

The excretion of urine was almost universally wanting during life ; and but little was found in the bodies of those who were examined, except, as was observed by a gentleman who had made many dissections, where the vomiting and purging had ceased for a long time before death.

From the dissections which were witnessed, and from the accounts of gentlemen* who had made a large number, it was inferred that the heart and large vessels did not generally contain any large quantity of blood. The heart was found sometimes empty, and sometimes all its cavities were moderately filled with blood. The arteries always contained black blood. On comparing blood found in the descending aorta with that contained in the corresponding part of the vena cava, that in the aorta resembled common venous blood, whilst that in the vein was still darker, thick and imperfectly coagulated, being nearly of the consistence and color of tar. In the cranium, there was an effusion of serum into the ventricles and at the base of the brain. The bloodvessels were quite full, but not unusually so. No morbid appearance was observed in the spinal nerve. The lungs were considerably congested. The stomach and small intestines exhibited a slight reddish tinge when held up to the light, but showed no signs of inflammation. The large intestines had a whiter or bleached appearance. There was no unusual dryness of the peritoneum. The whole canal was filled with the peculiar liquid matter which constitutes the evacuations in this disease. This was generally in large quantities, of a dirty greyish white color, though

* Dr. Merrill, of the Bellevue Hospital, informed us, that of the first twenty fatal cases all were examined after death.

in one case tinged with green, and of a flocculent appearance; sometimes quite thin, sometimes as thick as thin hasty pudding. It resembled gruel which has not been sufficiently boiled, or coagulated milk, the curd of which has been very finely broken up. Similar evacuations are occasionally witnessed in severe cases of common bowel complaints. Not a particle of fecal matter, or matter colored by bile, was noticed in any case, unless indeed the green color just mentioned was occasioned by the presence of bile. The gall bladder always contained bile, and its duct was pervious.

It is a matter of some interest and importance to determine whether we have any reason for believing, as some have done, that the spasmodic or malignant cholera is merely a more intense form of common cholera morbus, and is produced by an accumulation and concentration of the ordinary causes of the latter disease. That they have many symptoms in common, is not to be denied. It is quite certain that the common exciting causes of cholera morbus, such as irregularity and excess in eating or drinking, great fatigue and exposure, may also operate as exciting causes of spasmodic cholera. But, on the other hand, the course which the complaint takes, and the character of the symptoms in which it terminates, render it probable that there is, previous to these exciting causes, some peculiar predisposition of an unknown nature existing in the constitutions of the whole population where it prevails. Our common cholera, in some cases, reduces a patient to a state of great and irrecoverable exhaustion. It is accompanied by severe cramp, cold extremities, weak and fluttering pulse, ending in death. Yet, in a parallel state of exhaustion, is the aspect of the subject the same? Do the peculiarities of complexion, the state of the extremities, and the character of the evacuations, correspond? In common cholera, also, the exhaustion seems to be commensurate with, and to be produced by, the vomiting, purging and spasms; in the spasmodic, on the contrary, there is no such correspondence. The most rapidly fatal cases are not always those in which the vomiting, purging and spasms have been the most violent. Some individuals fall into the state of collapse almost at once, after but a short continuance of the symptoms which usually precede it.

It is desirable also to determine whether it be possible, in the earliest stage of spasmodic cholera, to distinguish it with certainty from an attack of the ordinary disease; to determine, for instance, in a place where cholera was not prevailing, that a case attended by vomiting, purging and spasms, was or was not the commencement of the epidemic. I fear the practitioner must wait for the symptoms attending the stage of collapse, before he can feel authorized to pronounce with certainty. In forming our judgment in such a case, we are to be chiefly governed by the state of the skin and pulse, and by the character of the evacuations. When the skin continues full of red blood, after considerable vomiting and purging; when the pulse remains full and of tolerable volume, and the extremities warm; when also the evacuations have a fecal or bilious appearance, one could hardly be mistaken in regarding the case as one of common cholera. Where, on the contrary, the pulse becomes quickly small and weak, with a dirty, dingy and bloodless skin, cool extremities and dejections of a light flocculent character, one would readily suspect

spasmodic cholera ; yet we surely see many such cases which the event, in ordinary seasons, proves to be nothing more than the common disease. Still, in such a case, if the malignant disease were either prevailing or expected, a physician could give no other than a doubtful opinion as to its character.

It did not appear, from such observation as was made of the effect of remedies, that any material variation was produced in the rate of mortality in cholera by the measures employed. This indeed seemed to be the general impression of those engaged in the management of the disease. And, it may be asked, has not this been the result, wherever it has prevailed, so far as we can judge from the reports of cases and deaths which we find in various publications ? We have had, it is true, many flattering recommendations of peculiar plans of treatment, and general statements of their efficacy ; but does not the general uniformity of the returns of dead and convalescent, in different places, satisfactorily show, that the good effects of remedies have chiefly existed in the imaginations of those who have employed them ? The probability is, that this epidemic, like all others, varies in severity in different places. This accounts for the apparently greater success of that method of treatment which happens to have been employed where the disease has been mild. So, too, when it first makes its appearance in a new spot, it seizes on the worst constitutions, and on persons most strongly predisposed ; and hence its great and appalling mortality. After a while it attacks individuals of better constitutions, and who are less strongly predisposed ; these make a more determined resistance to the disease, and recover perhaps in a greater proportion. Hence, towards the close of the epidemic, the cases seem to be more tractable, and to be more under the influence of remedies.

How can we, except by means of some such explanation as this, account for the apparent success which has attended modes of management the most opposite in their character, unless we believe the statements which are made to be wholly without real foundation, and to have had their origin in the want of accurate observation, the self deception, or the wilful misstatement, of their authors ? Many physicians are loose observers, many are loose reporters, and some are both. I know not in what other manner we can account for the assertion of Broussais, that he loses but one patient out of thirty, while all his medical brethren are losing half, or very near it. One might expect, if his statement were actually true, that public opinion would soon force the whole faculty to the adoption of a method of management so successful ; and that a Paris mob might imagine the physicians of all the hospitals, except that of the Val de Grace, engaged in the combination to poison the people—since in the latter the patients all came out alive, and in the former all dead.

It is unquestionably a humiliating confession to the medical art, that fifteen years' experience has not taught us any mode of arresting the destroying progress of this disease. Yet, if it be true, it is better that we should know and acknowledge it ; since then, instead of being distracted by the claims of opposite and contradictory statements, we shall direct our attention to the devising of new methods of treatment, or at least to means of prevention. The plague, which was once the scourge of Europe, is no more within the control of medicine, than it was centuries ago ; but it has been banished from countries which it once visited, by preventive measures. The same is true of yellow fever, and the same may be found true of cholera.

According to this view of the subject, the treatment of cholera cannot

yet be reduced to any fixed rules, but must be, in the main, tentative or experimental. It is not the intention of the writer, in speaking of means of treatment, to offer any opinion as to their probable efficacy, but merely to direct the attention to such as appear worthy of a *very thorough* trial ; for it is obvious that only the *very thorough* trial of a remedy gives it any fair chance of success in a disease like this.

We may also remark, that the rapid course taken by this disease does not allow us to place dependence on remedies which require time to produce their effect. Our whole range is confined to a very few hours. Except, therefore, in the premonitory stage, we must confine ourselves to means which operate almost immediately.

We should also bear it in mind, in treating cholera, that, in all cases of violent action or of extreme want of action, the susceptibility of organs to the influence of remedies is either very much exalted, or very much diminished ; generally the latter. Thus in fainting from excessive hemorrhage, great quantities of stimulus are required to produce an effect, and they must be frequently repeated in order to keep up the effect. Large doses of laudanum are also borne without the production of its usual operation. The same is true of any violent pain, and of excessive secretions. The power of the medicine given, is neutralized by the disease. Thus a man with diabetes will bear twenty or thirty grains of opium in a day ; and one with severe colic, two or three hundred drops of laudanum in a few hours.

It should also be premised, that the remarks made with regard to treatment refer to the confirmed state of the disease, that, viz. in which its peculiar character is fully developed. There is a premonitory or preparatory stage, in which the state of the system and the symptoms of disease are different, and require different management. It is not, however, in this stage that patients are generally seen in hospitals, nor usually in private practice ; but it is in this stage that many physicians are so sanguine with regard to the effects of remedies.

Whatever be the variety of internal means recommended by different practitioners, they almost uniformly agree in the propriety of external warmth and stimulus ; and in all diseases attended by coldness and want of action in the surface and extremities, the restoration of warmth and circulation is one of the first objects which suggests itself. In cholera, this is very strongly called for, since not only are the external parts cold and inactive, but, as some assert, the internal also. More proof, however, is required of the coldness of the internal organs than has yet been given : there are circumstances which render it doubtful, and it is a point which should be carefully investigated. Still, no doubt can exist of the coldness of a considerable part of the mass of the body.

Now it is very true, that this coldness is one of the consequences of the morbid condition on which the disease depends, and not the morbid condition itself, and that removing this effect will not prove a remedy for its cause. Yet it is also true, that many of the secondary effects produced by disease, are an obstacle to the removal of the disease, and obstruct the salutary efforts of nature or the influence of remedies. We often assist nature, and art also, in the struggle with the primary cause of disease, if we can artificially remove or suspend these secondary effects. Thus we assist the cure of dyspepsia by neutralizing the acid generated in the stomach as a consequence of this disease. So, too, where the system is sinking from a poison which operates by a suspension of that influence of the brain, which is necessary to respiration, if artificial

respiration be kept up for a sufficient time, the effect of the poison ceases, and life is preserved. Something like this may be true with regard to the power of maintaining the animal heat in cholera. The reduction of the temperature of a large portion of the body and circulating fluids, for several hours, would alone be sufficient to cause death, were the system otherwise capable of struggling with and overcoming that internal state in which the disease consists. If a man, with the ordinary power of maintaining animal heat, were exposed to a degree of cold which should reduce his temperature to 75 degrees, this reduction alone would soon destroy life. This often happens in cases of shipwreck and exposure at sea, where persons are chilled to death by immersion for a long time in water at a low temperature. The patient with cholera is placed in circumstances somewhat similar. His power of resisting cold being lost, he is cooled down by an ordinary atmosphere as much as a healthy man by the low temperature of the ocean.

A resemblance has been supposed to exist between the patient with cholera and an individual frozen by exposure, and it has been recommended to employ in the former the same treatment as in the latter case. But the resemblance is not so close as that which has been already suggested. In persons frost-bitten, the external parts are actually frozen; or at any rate reduced to a much lower temperature than those cholera patients, whilst the internal parts retain their powers of resistance. This at least is true of recoverable cases. Although the temperature to which the surface is reduced is much lower, yet the whole body has not been equally cooled, and the heat providing powers not equally exhausted. No one would think of dashing cold water, or rubbing melting snow, over the body of a man chilled by immersion in cold water, to restore his animal heat; neither is it probable that this measure would be attended by any beneficial result in cholera.

We may regard it, then, as an essential part of the treatment of cholera, whatever means we may otherwise employ, that warmth of the body should be restored by external heat, and its activity promoted by external stimulants. It is by no means a matter of small importance by what agents we effect this. Let us recollect what we are to accomplish, viz. to warm through a solid mass of flesh. In order to impart heat, we must in the first place surround the body with bad conductors, which will retain the heat which is communicated to it, viz. with blankets, rugs or comforters. We must, in the next place, apply, within these, substances which contain a good deal of heat, and which will give it out readily to the body; such as bottles of hot water, hot bricks, billets of wood, bags of sand, &c. &c. We may see at once how insufficient air baths must be, as indeed they have been found. Air is a slow conductor, and contains but little heat. How long would it take to raise the temperature of a dead body twenty degrees in an air bath? I suspect many hours. Hot air may warm the skin readily; but in cholera we must go deeper than this—we cannot rely on the heat-making power of the system to aid us in our endeavors—we must use means which shall extend as far as the coldness extends. Neither can we expect any amount of external covering alone to raise the temperature of the body. Blankets assist in the accumulation of heat, when the body is capable of generating it; but they in no degree tend to warm it, when it has not this power. They could never warm a drowned man who had ceased to breathe.

The consideration that the heat-producing power is suspended in the cholera patient, should lead us to be cautious in relaxing the application

of external warmth. It is not sufficient that the patient feels warm; the means should be persevered in, till a decided reaction has taken place, indicated by the state of the pulse, countenance and respiration, and should even then be cautiously relaxed. Neither should these measures be delayed till the patient is actually cold, when the stage of collapse is coming on. The loss of heat should, as much as possible, be prevented, by a great abundance at least of external covering, if not the application of absolute heat.

At the same time that the external warmth is thus provided for, it is important that the means used should not in some measure defeat their own object, by depriving the patient of fresh and cool air. Any imperfection of respiration, essentially impedes the keeping up of the animal temperature; and both foul air and warm air, are productive of such imperfection. We should be cautious, therefore, that the rooms of cholera patients be not unduly heated, and that an abundance of fresh air should always have access to them.

External stimulation seems indicated, as next in importance to external warmth. This might be effected in various ways; but none seems more speedy and certain in its operation, than the poultice, or plaster of flour of mustard and vinegar. How extensively it might be proper to apply this, experience only can teach us. Few, even of cholera patients, are for any long time insensible to its effects.

A powerful, and, it is believed, a new method of exciting reaction by external application, has been adopted by Drs. Lee and Roe, at one of the New York Hospitals, and, as has been stated, with remarkable success. It consists in the friction of the whole surface of the body, when in the state of collapse, with an ointment, composed of mercurial ointment, camphor and capsicum. Very little else is done, and nothing but a small quantity of drink, or of ice, given internally. We shall no doubt derive, ere long, a full account of the particulars of this mode of practice, and of the success which has attended it, from these enterprising physicians themselves; and it would be premature to give, at the present time, anything more than this general statement. It is certainly, however, one of the measures which deserve a thorough trial from those engaged in the treatment of cholera.

But little can be said of internal remedies. The general impression seems to be, both at New York and in Canada, that in the confirmed disease, nothing has as yet produced any very decidedly favorable effect, although all methods of practice have been tried; the calomel practice, the bleeding practice, the ultra-stimulating practice of spotted fever, the moderately stimulating practice, the camphor practice, and the ice practice. Still, we are hardly ready to give up in despair, and may inquire, whether it may not still be worth while to go over the ground again with some of these measures, unless we should be so happy as to escape a visit from this disease. The measures to which it might be desirable to give this trial, would be,—

1. The highly stimulating practice of Drs. Miner and North, by means of immense quantities of opium, brandy, capsicum and essential oils. So far as tried, this course has been said to be attended with unfavorable effects. Might it not bear a fairer chance of success, if combined with the injection of a saline solution into the veins? It may be, that these stimuli fail of their accustomed effect, from the want of a sufficient amount of circulating fluid.

2. The mercurial practice, carried to the greatest possible extent, both

internally and externally. Dr. Chisholm administered immense quantities of mercury to his yellow fever patients; one patient having used over 5000 grains, and many having actually taken more than a thousand grains by the mouth. We might do the same in bad cases of cholera, at least without danger; and besides the administration of calomel by the mouth, and frictions, we might also make use of mercurial fumigations continued for a great length of time, a whole day for instance, which would not only act mercurially, but would also assist in raising, or at least in maintaining when raised, the animal heat.

3. The administration of ice internally, according to the method of Broussais. Though not placing unqualified reliance on the accounts given by this teacher of his practice, and believing him to be much wanting either in accuracy or in sincerity and good faith, the peculiar circumstances in which we are placed with regard to the management of cholera, renders it our duty to employ all those means for which very decided success has been claimed.

4. Bleeding from the general circulation. It is remarkable that no remedy has been more strongly recommended than this, in works on cholera as it has elsewhere prevailed; and yet that no decided success has followed its employment in this country, so far as we have any evidence. Still we are led to believe, such is the authority on which it has been recommended, that there may be states of the disease in which it will be followed with advantage.

5. Injection of large quantities of warm water, or of a warm saline solution, into the veins. So far as we have been informed, the immediate effect of this measure has been to restore the circulation and warmth of the patient; in fact, to rouse him from the state of collapse. Of its final success, we know less. It is remarkable that, of all the cases mentioned in a late communication in an English Journal, and republished in the Boston Medical and Surgical Journal, we are not told of the result of a single case. It was employed in the case of a patient at the Bellevue Almshouse, at New York, by Drs. Morell and Baker, and twenty-four ounces of warm water were introduced into the veins. The immediate effect was highly promising, but relapse and death ultimately took place. We have been since informed that an instance of complete recovery has followed the saline injection, in the Crosby Street Hospital, under the care of Drs. Rhineland and Dekay. Measures which afford even a temporary revival should, in this disease, be seized on with eagerness. If therefore the injection into the veins proves to be as effectual in the promotion of a temporary reaction as has been asserted, it may answer a valuable purpose by prolonging life, and thus affording time for the operation of other remedies, even if it should not be sufficient for the preservation of the patient. Like external heat, therefore, its employment should be cautiously combined with that of all the other remedies in different cases; since we may, perhaps, arrive at that success from the judicious combination of means, which we seek in vain from perseverance in any single course.

Sickness at the State Prison in Charlestown.—A disease broke out very suddenly among the convicts at this place on Sunday afternoon last. We copy the following from the Boston Transcript of Monday evening.

Our correspondent of the Bunker Hill Aurora writes us under date 1 o'clock, P. M.:

"A sickness commenced at the State Prison in this town yesterday afternoon. At half past 12 o'clock, last night, 25 had been attacked severely, with vomiting and purging, but no spasms of the extremities. The number of cases has continued to increase, and there are now 104 sick—many of them, however, are in a state of convalescence. The patient most sick is pronounced to be improving. There have been no DEATHS; and the general opinion of our own and of the Boston physicians appears to be, that the disease is NOT the Asiatic Cholera. A Board of consulting physicians has been appointed, and also a committee of physicians to analyze the food and drink taken by the convicts yesterday."

The Inspectors of the prison visited it this morning, in company with the consulting and other physicians of the City and Charlestown. We learn that the physicians are of opinion that no case exists that is likely, from present appearances, to terminate fatally.

The Cholera.—The disease, in the city of New York, has much abated. The number of new cases and deaths on each day, since our last report, has gradually diminished, and on Friday last the former was 90, and the latter 24; on Saturday, cases 88—deaths 30. Drs. Arnold, Nelson, and Vought, have died of cholera in New York.—In Albany, the epidemic appears to have increased. On the 28th ult. there were 28 new cases and 18 deaths; on the 29th, 35 cases and 17 deaths; on the 30th, 26 cases and 10 deaths.—In Brooklyn, L. I. on the 1st inst. 35 cases and 7 deaths.—Cases have occurred in many towns on and near the Hudson river.—In Philadelphia, whole number of cases from the 27th July, 144; deaths 60.—In Providence, R. I., 4 cases have occurred.—The disease has broken out in another family in Newport, R. I. Mr. Foster, the father of the family, buried the two young ladies who died of cholera ten days since. After the interment, he returned home and slept with a child, who was taken sick on the 3d inst. and died the next morning. A few hours after the death of the child, the mother was seized in a violent and suspicious manner, and expired before 10 o'clock on the 5th. Another child has been taken, but is now convalescent. A few days after the interment referred to, Mr. F. was himself attacked with violent vomiting, &c., but his disease rapidly yielded to medicine.—The disease has also appeared in Norfolk, Va.—Boston still continues healthy.

Notice.—It was the intention of the Publishers of the Medical Journal, at the request of a number of subscribers, to have discontinued the plan, adopted the last year, of dividing the work into two volumes annually, and to have restored the original form of a single volume in a year. The call for the numbers of this volume, however, has been so much greater than was anticipated, that many of them are entirely taken up, so as to render it impossible to furnish the volume entire to any future applicants. In consequence of this, the 6th volume will close with the present number, and the next will begin volume 7. The title page and index will be sent with a future number.—A continuance of the favors of the profession, both as subscribers and contributors to this work, is respectfully requested.

The important nature of the communication respecting the Cholera in New York, has induced us to give it an insertion, entire, to the exclusion of the editorial matter prepared for this number. The proposed remarks on the management of the promontory symptoms of the disease, by the same writer, will be thankfully received.—The interesting Case by Dr. A. of N. H., and Prof. T.'s Experiments, together with other favors, will receive early attention.

Whole number of deaths in Boston for the week ending Aug. 4, 25. Males, 19—Females, 13. Still-born, 1.

Of scarlet fever, 5—diphtheria tremens, 1—consumption, 5—debility, 1—typhus fever, 1—scrophula, 1—dyspepsy, 1—liver complaint, 1—lung fever, 1—palpitation of the heart, 1—dysentery, 1—intermittent fever, 1—intemperance, 2—throat distemper, 1—abscess, 1—paralytic, 1.

Printed and published by CLAFF & HULL, 134 Washington Street, Boston.—\$3 a year, in advance.